

Incentives for preventing smoking in children and adolescents (Protocol)

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[Intervention Protocol]

Incentives for preventing smoking in children and adolescents

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ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

To assess the effect of incentives on preventing children and adolescents from starting smoking. Our review will address the following questions:

1. Do incentives prevent children and adolescents starting smoking?
2. Does the amount and type of incentive affect prevention of starting smoking?
3. What are the cost implications to the community of incentives?
4. Are incentives more or less effective in combination with other interventions to prevent starting smoking?
5. What are the unintended consequences arising from the use of incentives e.g. false claims, ineligible applicants?

BACKGROUND

Description of the condition

Currently, 1 in 10 deaths among adults worldwide can be attributed to tobacco use. This equates to more than five million people a year, making it the leading preventable cause of death globally (Mathers 2006). Global projections of mortality data estimate that unless urgent action is taken on tobacco control, this death toll will rise to more than eight million by 2030 (Mathers 2006).

Data from the Global Youth Tobacco Survey (2000-2007) have revealed that approximately 10% of students aged 13-15 years smoked cigarettes, from 5% in the Eastern Mediterranean to 19% in the European Region (Warren 2008). In the United States, surveillance data from 2007 found that 20% of grade 9-12 students smoked cigarettes, with no significant difference between boys (21%) and girls (19%) (Eaton 2008).

Adult smoking usually has its roots in adolescence. If individuals do not take up smoking during this period it is unlikely that they ever will (Mayhew 2000). Moreover, once smoking becomes established, cessation is challenging; the probability of subsequently quitting being inversely proportional to the age of initiation (Breslau 1996). Unfortunately, most smokers initiate the behaviour before 18 years of age; indeed among those who smoke cigarettes, nearly 25% of young people have reported smoking their first cigarette before the age of ten years (GYTS 2002).

Earlier onset of smoking provides for more life-years of tobacco use, thereby increasing the associated health risks, including respiratory conditions, cardiovascular disease and cancers (USDHHS 1994). Earlier onset is also associated with heavier use and heavier tobacco users are less likely to quit smoking and therefore more likely to experience tobacco-related health problems (Breslau 1996).

Given the prevalence of tobacco use among young people and the corresponding health burden into the future, strategies to prevent smoking in adolescence are a public health priority. Currently, public health policies and programmes aimed at reducing tobacco use among adolescents have demonstrated varying levels of success. Tax increases on tobacco products are successful in reducing smoking among this target group (USDHHS 1994) and there is some evidence for the effectiveness of mass media campaigns (Sowden 1998). Conversely, there is limited evidence for the effectiveness of community (Sowden 2003) and school-based programmes in reducing adolescent smoking (Thomas 2006).

One novel approach to reducing the prevalence of smoking is the use of financial incentives. While the evidence reviewed to date has involved the use of incentives as cessation interventions among adult smokers (Cahill 2008b, Cahill 2008a), there is growing interest in the use of similar incentives schemes to encourage young people to adopt healthy and pro-social behaviours (Kavanagh 2006). A review of financial incentives programmes to improve health or social behaviours in youth aged 11-19 years identified nine studies

which focused on healthy behaviours (Kavanagh 2006). A meta-analysis of these studies found a statistically significant positive impact, although the number of studies was small, as were some of the sample sizes.

Description of the intervention

Financial incentives may take the form of contests, competitions, incentive schemes, lotteries, raffles, and contingent payments. This range of incentives has previously been reviewed for their effectiveness for encouraging cessation and continued abstinence in smoking cessation programmes.

A Cochrane review of 'Quit and Win' contests found they delivered quit rates above baseline community rates, however the population impact appeared relatively low (Cahill 2008a). A separate Cochrane review of the use of competitions and incentives for smoking cessation found no evidence for the effectiveness of these interventions to enhance long-term abstinence from smoking, with any early success usually dissipating when the reward was no longer on offer (Cahill 2008b). The authors of both reviews noted the lack of high quality trials limited their conclusions and in the case of Cahill 2008b, most incentives in the included studies were small. On the other hand, a recent study of large financial incentives (up to \$750) in employees of a multinational company based in the United States found smoking cessation in the incentive groups was significantly greater than in the control group (Volpp 2009).

Financial incentives schemes have also been used for managing chronic conditions, avoiding sexually transmitted infections, weight loss and in education (Marteau 2009). Systematic reviews of the wider literature relating to financial incentives for encouraging healthy behaviours have found that incentives are effective in stimulating 'simple,' discrete behavioural changes (e.g. clinic attendance) (Jochelson 2007; Kane 2004). Incentives aimed at more complex lifestyle behaviours (e.g. smoking and sexual behaviour) are successful in increasing participation in health promotion programmes but once the incentive ceased, participants tend to revert to former behaviours (Jochelson 2007). It has also been argued that the size of the incentive is important, with higher-value incentives more powerful in encouraging behaviour change and participation in lifestyle programmes (Jochelson 2007).

How the intervention might work

Financial incentives operate on learning theory principles by giving an immediate reward for behaviours that will provide health gains in the future. In the field of behavioural economics, research has found that people are motivated by the experience of past rewards and the prospect of future awards (Carmerer 1999). Moreover, the desire to avoid regret (i.e. not being rewarded) can be a strong force in decision making under risk (Connolly 2006).

Incentive schemes are also framed around what is termed “present bias,” a tendency of humans to pursue immediate rewards ahead of rewards that are distant but more highly valued (Volpp 2008). Marteau et al. (Marteau 2009) notably highlight some unintended consequences of financial incentives, including the undermining of a participant’s intrinsic motivation (Kane 2004) and informed consent, as well as the potential for damaging the trust between health professionals and their patients.

Why it is important to do this review

While there is currently limited high quality evidence to support the use of financial incentive for smoking cessation, the two Cochrane reviews performed to date only included studies which targeted adults with the express aim of increasing quit rates. It is conceivable that financial incentives may be more successful with a young audience, who may be more sensitive to monetary rewards, and who might find it easier to not start smoking compared to the more complex task of quitting once addicted to nicotine. While there is promising evidence that incentives for youth might work, currently we do not know whether financial rewards are effective in preventing youth from starting to smoke. Given the magnitude of the problem globally and the lack of evidence to support other health promotion programmes in this area, this is an area worthy of further investigation.

OBJECTIVES

To assess the effect of incentives on preventing children and adolescents from starting smoking. Our review will address the following questions:

1. Do incentives prevent children and adolescents starting smoking?
2. Does the amount and type of incentive affect prevention of starting smoking?
3. What are the cost implications to the community of incentives?
4. Are incentives more or less effective in combination with other interventions to prevent starting smoking?
5. What are the unintended consequences arising from the use of incentives e.g. false claims, ineligible applicants?

METHODS

Criteria for considering studies for this review

Types of studies

Randomized controlled trials allocating individuals, groups or communities to intervention or control conditions. Controlled trials with baseline measures and post-intervention outcomes.

Types of participants

Children (aged 5 to 12 years) and adolescents (aged 13 to 18) in any setting. We will exclude trials aimed exclusively at pregnant women, since they are covered by the review *Interventions for promoting smoking cessation during pregnancy* (Lumley 2009) produced by the Cochrane Pregnancy and Childbirth Group.

Types of interventions

Contests, competitions, incentive schemes, lotteries, raffles, and contingent payments to reward not starting to smoke. We will include rewards to third parties (e.g. to schools, health-care providers or family members), as well as interventions that directly reward children and adolescents.

For each study, we will determine whether the participants received any other smoking interventions such as smoking education in school, and whether the control group received any interventions.

Types of outcome measures

Primary outcomes

The primary outcome is the smoking status of the children or adolescents who reported no smoking at baseline. We will use the outcomes defined by the included trials, but we will prefer sustained abstinence from smoking over point prevalence of smoking, where both are available. We will report smoking status at the longest follow up, and will require a minimum follow up of 6 months from baseline. We will prefer but not require biochemically validated abstinence over self report.

Secondary outcomes

We will assess the dose response of the amount of incentive. We will also record and assess the costs, and any harms from the use of incentives.

Search methods for identification of studies

Electronic searches

We will search the Cochrane Tobacco addiction Group Specialized Register, which includes studies identified by systematic electronic searches of multiple databases, and handsearching of specialist journals and the ‘grey’ literature (conference proceedings

and unpublished reports not normally covered by most electronic databases).

In addition we will search four electronic databases, MEDLINE, EMBASE, CINAHL and PsycINFO.

The following search terms will be used: incentive*, competition*, contest*, lotter*, raffle*, reward*, prize*, voucher*, gift*, inducement*, contingent payment*, deposit contract* in combination with terms for smoking and tobacco use, and children and adolescents.

Searching other resources

We will check cited studies while reviewing trial reports, and will contact trial authors for any required unpublished data. We will not apply any language restrictions.

Data collection and analysis

Selection of studies

One reviewer will prescreen all studies identified in the electronic search. Articles will be rejected at this stage if the title and/or abstract does not focus on the impact of financial incentives on adolescent smoking behavior. If the article cannot be categorically rejected by one reviewer, the full text will be obtained and screened by two reviewers.

Two reviewers will independently assess the relevant studies for inclusion. Discrepancies will be resolved by consensus and we will note reasons for the non-inclusion of studies (these will appear in the Table of Excluded Studies, with the reason for their exclusion). The Cochrane Tobacco Addiction Group editorial team will resolve any ongoing disagreements between the two reviewers.

Data extraction and management

Two reviewers will independently extract the following data into a data extraction form. At this point we will evaluate the quality of the data:

1. Study design, including inclusion and exclusion criteria, method of randomization (If used)
2. Setting (e.g. country, multi-centre or single centre, inpatient or outpatient etc.)
3. Demographics of participants, including average age, sex, socioeconomic status, smoking status
4. Intervention and control description
5. Outcome measures, including definition of abstinence and length of follow up, measurements used including any biochemical verification
6. Quality of report: presence of randomization, sequence generation, allocation concealment, blinding, intention-to-treat analysis, drop-out rates and whether missing data are balanced for intervention and control groups.

Assessment of risk of bias in included studies

The two reviewers will assess the risk of bias by including their judgement in the data extraction table of the following:

1. Was the sequence generation adequate?
2. Was allocation concealed?
3. Who was blinded?
4. Were incomplete data addressed? (e.g. Was there an intention-to-treat analysis? Was attrition greater than 20%? And was there differential attrition between intervention and control groups?)
5. Was the study free of selective reporting? (e.g. were all of the study's pre-specified outcomes reported?)
6. Was the study free of detection bias? (e.g. was there biochemical verification of self-report smoking status?)

Measures of treatment effect

We will aim to provide a risk ratio (RR) for the outcome for each trial, defined as (number who were smokers in the intervention group / total number randomized to the intervention group) / (number who were smokers in the control group / total number randomized to the control group). The RR will be less than 1, and favour the intervention, if more participants abstained from smoking in the intervention group compared to the control group. We will calculate an estimated pooled weighted average of RRs, using the Mantel-Haenszel fixed-effect method, with a 95% confidence interval.

Unit of analysis issues

Adjusted RRs from cluster-randomized trials using schools as the unit of analysis will be obtained either directly from those trials that report adjusted results or by adjusting the original (non-adjusted) risk ratios using an intraclass correlation coefficient (ICC) of 0.097 (the ICC for current smoking status averaged among all ethnicities reported by Siddiqui et al. (Siddiqui 1996)). This will allow for the pooling of both cluster- and individually randomized trials. Adjusted RRs from cluster-randomized trials using groupings other than schools (e.g. neighbourhoods) as the unit of analysis will be obtained either directly from those trials that report adjusted results or by adjusting the original (non-adjusted) risk ratios using an appropriate intraclass correlation coefficient (ICC) for that grouping or, if that is not available, analysed separately.

Dealing with missing data

Where possible we will contact the trial authors to request missing data. We will exclude participants for whom no outcome data is available, rather than conducting an intention-to-treat analysis of all randomized participants with imputed values for the missing data (Higgins 2008).

Assessment of heterogeneity

The method of synthesising the studies will depend on the type, quality, design and heterogeneity of the included studies. We will consider pooling the data in the event that no significant heterogeneity between the studies is demonstrated. We will use the χ^2 test and the I^2 statistic to assess statistical heterogeneity. An I^2 value of greater than 50% may be considered to represent substantial heterogeneity (Higgins 2008).

Assessment of reporting biases

If there are at least 10 studies included in the meta-analysis, we will prepare a funnel plot to investigate for the possibility of reporting biases.

Subgroup analysis and investigation of heterogeneity

If there is substantial heterogeneity present, we will explore the reasons for this, which may include undertaking subgroup analyses (if there are sufficient studies to do this). Possible subgroup analyses may include: by type of intervention (solely financial rewards versus financial rewards plus other smoking cessation intervention; staged versus one-off incentive); type of incentive (individual versus rewards to third parties; lottery versus definite payment of a specified reward amount); size of the incentive (low, high).

Sensitivity analysis

We will test study design in a sensitivity analysis by first including, then excluding less rigorous trials (e.g. quasi-randomized trials).

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* Indicates the major publication for the study

HISTORY

Protocol first published: Issue 8, 2010

DECLARATIONS OF INTEREST

None known.